Letters

Weathered Bedrock in New Jersey

The article entitled "Recent saprolite" that appeared in a recent issue of *Science* (*1*) is, in my opinion, an example of an extreme hypothesis and should, somewhere, have carried a statement to that effect. If the interpretation made by Minard regarding the length of time involved in the production of a thick saprolite in northern New Jersey should happen to be correct, a great deal of doubt could be cast on the use of the degree of soil development as an index for estimating relative amounts of elapsed time during the Cenozoic era. For this reason, as well as because many nongeologists may not be aware of the controversial possibilities in the article, I feel compelled to point out several arguments advanced by Minard that I believe to have resulted from faulty reasoning.

In order to support his hypothesis of exceedingly rapid weathering of the Pochuck gneiss at this place, Minard suggested that glacial abrasion would have removed it completely or would have greatly distorted the layering in the saprolite. He also argues that had it been overridden by the glacier while frozen, a congeliturbate structure should be found in the upper part of the material. Neither of these postulates is valid. Near its margin, an ice sheet can readily move over unconsolidated material without removing or distorting it. The ice lobes that passed through the Great Lakes basins buried many nearly complete soil profiles virtually undisturbed as much as 15 to 20 miles back from their margins (*2*); this saprolite locality is only 5 miles north of the glacial boundary in New Jersey.

Even though Salisbury's classic report on the glacial geology of New Jersey (*3*) was published in 1902, I am sure that it is not so obsolete that it can be disregarded completely. Salisbury stated that locally, near the last glacial boundary, till rests on disintegrated rock; in every observed case, relationships of the materials indicate that the ice had failed to remove the weathered rock. Similar statements were made in the Raritan folio (*4*).

Salisbury also reported that although striae are rare, those found on exposed bosses of gneiss in this vicinity indicated a direction of ice movement of about S 12°W. If these data can be accepted, this particular saprolite-covered ridge actually is 300 feet below the crest of a hill and partly protected on the lee side, one of the better places to search successfully for buried soil profiles in a glaciated region. Structures that might be called congeliturbates are rare near the southern limits of glaciation in Illinois, Indiana, and Ohio, although they have been reported from Pennsylvania (*5*).

MacClintock's study of the degree of weathering of gneissic cobbles in the drift of northern New Jersey was an attempt, which seems to have been reasonably successful, to relate differences in the degree of weathering of certain types of cobbles in the pre-Wisconsin drift to the age of the enclosing glacial sediment (*6*). The presence of weathered cobbles in unweathered ice-laid drift can be readily explained if one understands that glacial ice picked up chunks of saprolitic material as it moved forward. It then incorporated some of these chunks into the sediment without wholly destroying them.

Hunt and Sokoloff (*7*) did not present "evidence for rapid, deep weathering." Rather, they pointed out that we are in no position to evaluate time as a factor in the development of a paleosol unless we know much more than we do now about the climate and other aspects of the environment that existed where any particular old soil was formed.

The existence of a thick saprolite on gneiss in one locality, covered by a very thin layer of weathered drift, should be an indication that this thick weathered zone on the gneiss did not form under the same conditions of time and climate that produced a far thinner weathered zone on the same materials nearby. The depth of weathering (that is, removal of carbonates) of the Wisconsin till in New Jersey is not great, generally 2 to 3 feet, and rarely as much as 5 feet (*3*). Minard points out that about 10 miles south of this locality the saprolite blanket on unglaciated gneiss is 60 feet thick. I would find it far easier to accept a hypothesis that the 25-foot-thick saprolite in the locality under discussion is part of a pre-Wisconsin soil the ice failed to dislodge. Minard's postulate that it is the result of weathering during the past 18,000 to 20,000 years seems to me untenable.

As minor editorial points on Minard's article, the dates of his references 4 and 5 are incorrect, as is the publication number of his reference 5. His

(*Continued on page 1719*)

Letters

(Continued from page 1678)

map (Fig. 1) is confusing unless one colors it or uses both the Raritan folio (1/125,000) and the Stanhope (N.J.) topographic quadrangle (1/24,000) for reference.

WILLIAM J. WAYNE
Indiana Geological Survey, Bloomington

References

1. J. P. Minard, *Science* **129**, 1206 (1959).
2. M. M. Leighton and P. MacClintock, *J. Geol.* **38**, 28 (1930); W. D. Thornbury, *Indiana Dept. Conserv. Div. Geol. Publ.* (1937); W. J. Wayne, *J. Geol.* **66**, 8 (1958).
3. R. D. Salisbury, *New Jersey Geol. Survey Publ. No. 5* (1902), pp. 451-460.
4. W. S. Bagley, R. D. Salisbury, H. B. Kummel, *U.S. Geol. Survey Folio No. 191* (1914), p. 18.
5. C. S. Denny, *Ohio J. Sci.* **51**, 116 (1951).
6. P. MacClintock, *Bull. Geol. Soc. Am.* **51**, 103 (1940).
7. C. B. Hunt and V. P. Sokoloff, *U.S. Geol. Survey Profess. Paper No. 221-G* (1950), pp. 117-118.

J. P. Minard presents some interesting observations with respect to the presence of deeply weathered bedrock immediately north of the Wisconsin glacial moraine in New Jersey. The author relates this deep-seated weathering to post-Wisconsin time. That the saprolite exists in the area is not questioned. Relating the weathering of the bedrock to post-Pleistocene time, however, raises some serious questions. The author's interpretation of when this weathering took place is in direct contradiction to publications of mine (*1–3*), and it does not agree very well with the bulk of the published opinion of surficial geologists (*4, 5*).

One feature which appears to be quite characteristic of soils on deposits of post-Sangamon age in the podzolic area is that the bottom of the B horizon is very distinct, grading abruptly into a C horizon of comparatively fresh material. Below the B horizon the minerals (*3*) and the morphology show few changes since deposition of the till. This unaltered C horizon is not only demonstrable in the field throughout the northeastern United States and southeastern Canada but has been well documented in countless publications on soils for nearly half a century.

With soils which pre-date the Wisconsin glacial stage, a deep-seated weathering has taken place to depths of many feet, often well into the bedrock. Soils which have undergone the long periods of weathering of the Yarmouth and Sangamon interglacial stages show only minor color variations between the B and C horizons, the C horizon having been highly altered (*2*).

If the weathering processes were deep-seated during post-Wisconsin time, why were they confined to one locality? Certainly major climatic changes were not so highly localized as to effect major alterations in one specific area while effecting no appreciable weathering below the solum in other, contiguous areas of Wisconsin glaciation. We cannot, on the one hand, speak of deeply weathered and differentially altered minerals of gneissic bedrock to a depth of 10 to 20 feet below the surface in one location and completely ignore the widespread persistence of unweathered carbonate and other minerals 2 to 4 feet below the surface in the same general area.

The author indicates that the lack of "congeliturbate structure" would preclude the probability that the regolith was frozen during glaciation. The absence of special structural conditions (induced by cryopedologic processes) in the soil in one locality in itself proves little. While it has been demonstrated (*5*) that cold-climate processes did operate to a degree in Wisconsin glaciated areas, field observations clearly show that these well-defined cold-climate structures are more commonly absent than present.

The author's arguments for rapid weathering at the site prove little, and the references cited have only indirect relation to the subject. It is unfortunate that the voluminous literature relating directly or indirectly to the lack of deep-seated weathering in deposits of Wisconsin age was not tied in with the article.

In addition to the site mentioned by the author, there are other, similar locations in New Jersey within the area of Wisconsin glaciation which show deep-seated weathering. These scattered atypical conditions appear to be confined to a belt a few miles wide immediately north of the Wisconsin terminal moraine. If the sites were glaciated, there must have been a minimum of glacial scouring. On the basis of regional soil morphology in the fringe areas of the Wisconsin-glaciated area, these scattered highly weathered soils appear to resemble more closely those of deposits of Illinoian and Kansan age (Annandale) than those of Wisconsin age (Rockaway).

While I disagree with Minard's interpretations as to when the weathering of the bedrock took place, his recording of the observation in itself represents an important contribution to Pleistocene research.

J. C. F. TEDROW
Soils Department, Rutgers University, New Brunswick, New Jersey

References

1. J. C. F. Tedrow and A. S. Wilkerson, *Soil Sci.* **75**, 345 (1953); R. D. Krebs and J. C. F. Tedrow, *ibid.* **85**, 28 (1958).
2. J. C. F. Tedrow, *Soil Sci. Soc. Am. Proc.* **18**, 479 (1954).
3. R. D. Krebs and J. C. F. Tedrow, *Soil Sci.* **83**, 207 (1957).
4. P. MacClintock, *Bull. Geol. Soc. Am.* **65**, 369 (1954); G. W. White, *ibid.* **53**, 1813 (1942); L. C. Peltier, *Penn. Geol. Survey Bull. G No. 23* (1949); R. D. Salisbury, "Glacial Geology," vol. 5 of *Geological Survey of New Jersey* (1902).
5. C. S. Denny, *U. S. Geol. Survey Profess. Paper No. 288* (1956).